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Morgan's Canon: Animal Psychology in the Twentieth Century and Beyond

An enormously important role has been played in twentieth century animal science by the principle known as 'Morgan's Canon', first formulated in the nineteenth century by Conwy Lloyd Morgan (1852-1936), a British ethologist widely regarded as one of the most influential founders of comparative psychology. The principle was expressed by Morgan over the course of his lifetime in a number of different ways, but the one which seems to be most often quoted in the literature is from the first edition of his *Introduction to Comparative Psychology*: "In no case may we interpret an action as the outcomes of the exercise of a higher psychical faculty, if it can be interpreted as the outcome of the exercise of one which stands lower in the psychological scale."¹ The influence that the principle has had on the conduct and methods of comparative psychology over the succeeding hundred and twenty years cannot be over-estimated; it has been described by one writer as "possibly the most important single sentence in the history of the study of animal behavior."² It was certainly implicated in the widespread adoption of radical psychological behaviorism during the middle decades of the twentieth century; but its impact has outlasted the demise of that doctrine. The Canon is still regularly explicitly invoked by scientists working on animal behavior and is perhaps even more often silently applied as part of an implicit orthodoxy concerning the appropriate methodology for a sober psychological science.³

However, the Canon has been severely criticized in recent years by some powerful detractors. Daniel Dennett⁴, Elliott Sober⁵ and Jerry Fodor⁶ have all argued, in different ways, that Morgan's Canon is either redundant, defensible only on a quite seriously modified interpretation of its meaning, or simply silly. A number of other philosophers have also contributed excellent critiques.⁷ Historians of science, moreover, have shown convincingly that the Canon has been utilized by scientists of animal behavior in ways that were never intended, and moreover, would have been deplored, in fact, by Morgan himself.⁸ The case against the Canon has been thoroughly made with considerable power, in more than one way, by a range of philosophers and historians. Why, then, write yet another paper on Morgan's Canon?

One reason for thinking that there is room for another nail or two in the coffin of the Canon is that, as already noted above, the principle appears as yet to be thoroughly undead in the

¹ C. Lloyd Morgan, *An Introduction to Comparative Psychology*, 1st edition (London and Newcastle-upon-Tyne: The Walter Scott Publishing Co. Ltd., 1894), 53.

² B.G. Galef Jr., "Historical origins: the making of a science", in *Foundations of Animal Behavior: Classic Papers with Commentaries*, eds. L.D. Houck and L.C. Drickamer (Chicago: University of Chicago Press, 1996): 9.

³ For examples of explicit invocation, see, for example, Robert Epstein, "The Principle of Parsimony and some Applications in Psychology", *The Journal of Mind and Behavior* 5 (1984): 119-30; Stephen Budiansky, *If a Lion Could Talk: How Animals Think* (London: Wiedenfeld, 1998); M. Rosalyn Karin-D'Arcy, "The Modern Role of Morgan's Canon in Comparative Psychology", *International Journal of Comparative Psychology* 18 (2005): 179-201; Sara J. Shettleworth, "Clever Animals and Killjoy Explanations in Comparative Psychology", *Trends in Cognitive Sciences* 14 (2010): 477-81.

⁴ Daniel Dennett, "Intentional Systems in Cognitive Ethology: the Panglossian Paradigm Defended", in *The Intentional Stance* (Cambridge, MA: MIT Press, 1993): 237-68.

⁵ Elliott Sober, "Morgan's Canon", in *The Evolution of Mind*, ed. D.D. Cummins and C. Allen (Oxford: OUP, 1998).

⁶ Jerry Fodor, "Not So Clever Hans" in *London Review of Books*, February 4, 1999: 12-13.

⁷ Of particular note are those by Simon Fitzpatrick, "Doing Away with Morgan's Canon", *Mind and Language* 23 (2008): 224-46; Sean Allen-Hermanson, "Morgan's Canon Revisited", *Philosophy of Science* 72 (2005): 608-31; and Martin Montminy, "What use is Morgan's Canon?" *Philosophical Psychology* 18 (2005): 399-414.

⁸ See, for example, Robert H. Wozniak, "Conwy Lloyd Morgan, Mental Evolution, and The *Introduction to Comparative Psychology*", 1997. <http://brynmawr.edu/psychology/rwozniak/morgan/html>. Accessed January 12, 2015.

disciplines to which it is of direct relevance. It still stalks the corridors of psychology departments, making its unholy presence firmly felt⁹; and moreover, though this is rarely noted, its influence also continues to be discernible well beyond the academy. The anthropomorphic tendencies of the general public, delighting to tell anecdotes supposed revelatory of the reasoning powers, emotional sophistication and general intelligence of their pets, have long been deplored by many Canonised practitioners of animal science but in fact, many people reveal, even as they offer up these anecdotes, an astonishingly high level of awareness of the possible perils of over-imaginative interpretation, often laughingly following their tales with embarrassed acknowledgements of the foolish anthropomorphisms they take to be thereby exhibited. One reason for this high level of awareness is that the cautious and purportedly objective descriptions of animal behavior which have been supposed part of a respectable scientific animal psychology have thoroughly permeated the broadcasts and publications by means of which the public gains much of its scientific understanding of the animal world. To take one instance, the banter between Chris Packham and Michaela Strachan on recent editions of *Springwatch* and *Autumnwatch*, BBC nature programmes which may be familiar to British readers, regularly involves the attribution of high-level psychological states to the creatures observed by the giddy Michaela, lover of animals, who is gently put right on the matter by the sober and biologically better-educated Chris, urging caution, and the strenuous avoidance of anthropomorphism.¹⁰ The message which often emanates from these and many other wildlife programmes, and from the language in which their descriptions of animal behavior are cautiously couched, is that anyone who knows anything about the science of animals knows also that their activity ought not to be interpreted by means of the tools which often seem to come naturally to human observers: the tools made available by intentional descriptions, and the assumption of such things as agency and perhaps even consciousness, in the creatures being observed. Real scientists, we are encouraged to think, know better. But do they know better? The extent to which Morgan's Canon may be responsible for a now thankfully teetering orthodoxy needs to be appreciated, before this question can hope to receive a serious answer.

A second reason for thinking that Morgan's Canon deserves another airing is that the debate about the principle is far less widely known outside psychology than it ought to be. Amongst philosophers of mind and action, for instance, the Canon is rarely mentioned. I had been working, broadly speaking, in the Philosophy of Mind for many years when I first heard of it, my attention directed to its influence and interest not by a fellow philosopher, but by a colleague working in the History of Science, Greg Radick, who had himself written about the Canon.¹¹ I suspect a poll of philosophers of mind would reveal similar levels of general ignorance. And yet the principle is enormously significant. The question whether it is to be endorsed or rejected is utterly central to the pursuit of questions about animal mind, to questions about other minds in general, to questions about humanity's place in nature. Philosophers of mind need to know about it, and they need to know about the influence it may have had on the psychological work from which they increasingly frequently take their inspiration. Not to know is to risk mistaking the products of various contingent historical factors for the non-negotiable outcome of the inevitable onward march of an increasingly objective science.

⁹ For affirmations that Morgan's Canon continues to play a useful role in psychology, see M. Rosalyn Karin-D'Arcy, "The Modern Role of Morgan's Canon"; Dominic M. Dwyer and Katy F. Burgess, "Rational Accounts of Animal Behaviour: Lessons from C.Lloyd Morgan's Canon", *International Journal of Comparative Psychology* 24 (2011): 349-64; Sara J. Shettleworth, "Clever animals and killjoy explanations".

¹⁰ See e.g. the discussion of reed warblers' inability to detect cuckoo chicks which have hatched in their nest, *Springwatch* 2014 Episode 10.

¹¹ Greg Radick, "Morgan's Canon, Garner's Phonograph, and the Evolutionary Origins of Language and Reason", *British Journal for the History of Science* 33 (2000): 3-23.

A third and final reason for thinking that more needs to be said about Morgan's Canon is that the existing literature is rather confusing. Not everyone who criticizes the Canon is criticising the same version of it; moreover, interpretations of its crucial terms differ, so that not everyone who appeals explicitly to the Canon really has quite the same principle in mind. Some points which are made in opposition to the Canon ought never to have been made, for they are very easily met, and ought to be discarded in favour of the more powerful arguments which are available. And in addition, one needs, of course, to give proponents of the Canon a fair hearing, and to respond to their anxiety that to lose the Canon might be to open the door to an absurd degree of anthropomorphic licence. Simon Fitzpatrick has argued that many endorse the Canon only because they fail to see that the work they believe it is needed to do, can be done instead by an alternative scientific principle, a principle which Fitzpatrick calls 'evidentialism'. Scientific evidentialism, in its most general form, simply states that in no case should one endorse any scientific theory or explanation if the available empirical evidence gives us no reason to prefer it to an alternative theory or explanation. Formulating a version specifically directed to the interpretation of animal behavior, Fitzpatrick offers the following:

<ext>"*Evidentialism*: in no case should we endorse an explanation of animal behaviour in terms of cognitive process X on the basis of the available evidence if that evidence gives us no reason to prefer it to an alternative explanation in terms of a different cognitive process Y – whether this be lower or higher on the 'psychical scale'."¹²</ext>

Evidentialism supports the view that in circumstances in which we genuinely have no proper empirical reason of any sort to prefer one of two theories to another, the correct response is *agnosticism*, together, perhaps, with a redoubling of efforts to ascertain (by means of experimentation or further observation) which of the two theories really does offer the better explanation.¹³ But *endorsement* of either theory until these efforts have been made successfully would go too far. And endorsement is what Morgan's Canon purports to justify. Evidentialism, then, opposes Morgan's Canon. And I shall concur with Fitzpatrick in arguing that it is evidentialism, and not the Canon, which ought to be the touchstone of good science.

In some ways, I confess, it just seems obvious to me that if we are looking for a quite general principle of theory choice in the realm of comparative psychology, evidentialism embodies a much safer scientific attitude than does Morgan's Canon. (In case this is not utterly plain, I shall offer in section (v) some examples which make clear that Morgan's Canon both can encourage, and has in fact resulted in the endorsement of unsafe verdicts on animal behavior). The interesting question is really how, and why, Morgan's Canon has managed to last so long. In an attempt to answer this question, I shall look at some of the arguments which might conceivably have convinced some of its proponents of its legitimacy, and will seek to undermine them. But the correct explanation of its tenacity may ultimately be one that cites causes rather than reasons. Morgan's Canon has been of

¹² Fitzpatrick, "Doing Away with Morgan's Canon", 242.

¹³ This crucial point – that in the absence of empirical reasons to prefer one explanation over another, agnosticism is the appropriate attitude, until further empirical evidence of the sort which might enable a rational preference to be formed – is made by many critics of the Canon, including Sober: "the canon seems to license inferences in which the proper conclusion is that no inferences can be drawn", "Morgan's Canon", 228; Fodor: "A priori, there is nothing to choose between the hypothesis that a certain behavior has a "higher" sort of mental aetiology and the hypothesis that it doesn't. if you want to know which is true, you just have to go and look" ("Not so Clever Hans", 12); and Fitzpatrick: "... if the worry about crude anthropomorphism is that it is not sufficiently supported by the data, it is not clear that we are thereby justified in endorsing the least cognitively sophisticated explanation available, since that explanation may not be sufficiently supported by the data either. After all, we could instead simply be agnostic ..." ("Doing Away with Morgan's Canon", 235).

the utmost importance to the attempts of psychology to escape such evils as anecdotalism and introspectionism, and to psychology's struggle to establish itself as a proper scientific endeavour, alongside the 'hard' sciences. To discard it may feel to the practitioners of that science like a tremendously dangerous step in an unwanted direction. In this paper, I shall add to the attempts that others have made to allay this concern. Like a mercenary once recruited for a good cause, but now exhausted and capable of being induced to fight for the other side, Morgan's Canon is now itself the danger to good science.

(i) *The Canon according to Morgan*

As already noted, Morgan's Canon states that "In no case may we interpret an action as the outcomes of the exercise of a higher psychical faculty, if it can be interpreted as the outcome of the exercise of one which stands lower in the psychological scale."¹⁴ The first duty of any philosopher tasked with arriving at a good understanding of Morgan's Canon is evidently the interpretation of the crucial terms 'higher' and 'lower'. But before embarking on an attempt to decide how best we are to understand the nature of the hierarchy which is implied by these terms, it is worth pausing to take note of a number of additions and clarifications which were made by Morgan himself.

One addendum which is made almost immediately by Morgan after stating his canon expresses a certain discomfiture at his own use of the word 'faculty'. Suspicion of "faculty psychology" was in the air in the late nineteenth century; Wundt had already criticized the misleading "mythological" conception of a psychological faculty as something which itself acts, in his *Grundzüge der physiologischen Psychologie*,¹⁵ first published in 1874. Morgan suggests as an alternative, for those who are worried by the use of the term 'faculty', a restatement of his Canon in terms of an ontology of processes: "In no case is an animal activity to be interpreted in terms of higher psychological processes, if it can be fairly interpreted in terms of processes which stand lower in the scale of psychological evolution and development."¹⁶ This restatement offers not only an alternative ontological framework for the understanding of the Canon, one which has tended to be found more appealing by modern practitioners both of psychology and philosophy than the faculty-based formulation, but also a clarification of the relevant notion of a "psychological scale". We are told here that the "scale" according to which the processes can be ranged in a hierarchy is a "scale of psychological evolution and development"; I shall turn presently to some possible concerns about this suggestion.

Another important elucidation is that when Morgan speaks, in the original formulation of his principle of "an action" ("in no case may we interpret an action ..."), he is speaking not of what contemporary philosophers would call act *tokens*, but rather of act *types*; so that the question is not whether, on a given occasion, we should attribute a piece of animal behavior to this or that faculty, this or that process, a decision which, as Morgan notes, would be "a purely individual matter of comparatively little moment,"¹⁷ given the independently evidenced assumption that the creature in question possesses both faculties (or utilizes both processes). The question is whether a type of activity *in general* – a pattern of activity which one sees repeated on a number of occasions across different individual members of the same species, or even across members of many species, in different times and places, such as web-spinning in spiders, the broken-wing behavior in certain

¹⁴ Morgan, *An Introduction to Comparative Psychology*, 53.

¹⁵ Wilhelm Wundt, *Grundzüge der physiologischen Psychologie* (Leipzig: Engelmann, 1874).

¹⁶ Morgan, *An Introduction to Comparative Psychology*, 59.

¹⁷ Morgan, *An Introduction to Comparative Psychology*, 54.

waders, plovers and doves,¹⁸ grooming in primates, and so on, and capable of being interpreted as the “outcome” either of a “higher” or a “lower” process – should, in the absence of any further, independent evidence, be attributed to the “higher” or to the “lower” one. Morgan is absolutely clear, moreover, that “it should be added, lest the range of the principle be misunderstood, that the canon by no means excludes the interpretation of a particular activity in terms of the higher processes, if we already have independent evidence of the occurrence of these higher processes in the animal under observation.”¹⁹ The principle applies only when one does *not* have such independent evidence. One may not, therefore, on Morgan’s view, use the Canon to insist upon “lower” interpretations of behaviors in certain animals if one already has evidence of relevant capacities in those animals which might permit “higher” interpretations to be more plausible.

What, then, should we make of Morgan’s talk of “higher” and “lower” faculties and processes? The first and most important point is that Morgan’s own distinction is a distinction amongst faculties/processes *all of which are “psychical”*. This is easy to miss, particularly in view of the widespread appropriation of the metaphor of hierarchically-ranked levels in contemporary thought to understand the relation between different sciences and their theories, explanations and vocabularies. Confusion on this score might well incline us to leap immediately to the conclusion that Morgan’s Canon tells us to prefer the explanations of the sciences ranked lower in this hierarchy (for example, those of neurophysiology or biochemistry), to those ranked higher (such as those of psychology), where they are available. But this was no part of Morgan’s own view. Morgan’s conception of psychology was organized in terms of a hierarchy of powers or faculties which might be invoked in the explanation of behavior, at the bottom of which was to be found “mere instinct”, and at the top, reason. The distinction between the two had already been discussed in the introduction to Romanes’ work, *Animal Intelligence*²⁰, and this discussion represents the starting point for Morgan’s methodological deliberations in his own *Introduction to Comparative Psychology*. Morgan’s distinctive advance on Romanes was to argue that between these two domains of psychic life, we must recognize ‘a vast field of animal and human activity which ... [should be] ... distinguish[ed] from both by the application of the term ‘intelligence.’”²¹ For Morgan, it is clear that we are going to need (at least) a threefold distinction between instinct, intelligence and reason for the interpretation of animal behavior. One thing which the Canon clearly entails for Morgan, then, is at least this: that what we can perfectly well interpret as the product of instinct, we should not seek to explain by means of the attribution of intelligence; and what we can understand as an exercise of mere intelligence, we should not consider to be a demonstration of reason.²²

¹⁸ Some species of bird defend their eggs and chicks by leading potential predators away from the nest with a display in which they mimic having an injury to a wing. Once the predator has been safely distracted, they are usually then able to fly off to safety themselves.

¹⁹ Morgan, *An Introduction to Comparative Psychology*, 59.

²⁰ George Romanes, *Animal Intelligence* (London: Kegan Paul, Trench & Co., 1882).

²¹ Conwy Lloyd Morgan, “The Limits of Animal Intelligence”, *Fortnightly Review*, Aug. 1893: 223-39, 225. Though Romanes had of course also utilized the word “intelligence” (witness the title of his major work), he appears to conceive of it rather as a rather lowly variety of reason, rather than, as Morgan has it, a set of powers deserving of its own distinctive category.

²² To a modern ear, it perhaps seems strange to think of explanations which invoke mere instinct as psychological explanations. The invocation of instinct has come to be seen in some contexts as something *opposed* to the invocation of mentality; and often also as something opposed to the existence of real agency in a creature. But this was not at all how the concept was understood by Romanes, from whose discussion, as already mentioned, Morgan’s takes its point of departure. For Romanes, instinct was to be clearly distinguished from mere reflex by “the element of mind” (*Animal Intelligence*, 11). Romanes is clear that instinct “involves *mental* operations” (*Animal Intelligence*, 11) – by which he means that a certain degree of consciousness is implied. He is sensible, of course, that in many instances it may be difficult or impossible to decide whether or not the mind-element is present – but his considered view is that “this is altogether a

It need hardly be said that the Canon's directive has not always been interpreted in this strictly intra-psychic way by many modern and not-so-modern psychologists. Very often, the Canon has been thought to sanction a preference for explanations which do not involve the attribution of consciousness over those which do; for explanations which are mechanistic rather than intentional; for explanations of behaviors in terms of such things as fixed action patterns, stimulus-response mechanisms, tropes and reflexes, rather than anything which might be better understood by reference to such things as judgement, decision, or even emotion – without any essential reference, indeed, to *anything* which might make the psychological grade. Of course, it is a vexed matter to say what it is for any kind of faculty or process to make the psychological grade but most philosophical attempts at defining the psychic have made some kind of reference or other either to consciousness or to intentionality, or both. Morgan's Canon has been assumed to justify the general privileging of the non-psychic over the psychic. But that is certainly not how Morgan intended it. Indeed, Morgan himself would certainly be turning in his grave if he knew how the Canon had been deployed within the science of comparative psychology of which he has a good claim to be regarded as one of the founding fathers. Following his discussion of animal memory, he makes the following request:

<ext>“I will only ask the reader ... to be good enough to credit me with an unbiassed (sic) desire to interpret the phenomena of animal psychology without exaggeration, either in the direction of excess *or defect* of mental power and differentiation. As an evolutionist who believes that the whole range of the mental faculties have been developed by natural processes, *the tendency of my bias would assuredly not be in the direction of setting a gulf between the faculties of animals and the faculties of man*”.²³ </ext>

Morgan himself is in the business of defending the principle that mind is likely to be found throughout the animal kingdom, though reaching different degrees of sophistication in different animals. A 'gulf', it is worth bearing in mind, is what he never would have countenanced. And yet a gulf is what the practical application of his Canon has often suggested.

(ii) *Extending the Canon?*

It might be said, of course, that the question what Morgan himself intended is really of only secondary and purely historical interest. Even if his was, as I have suggested, intended to be an intra-psychic scale, it might be alleged that there is no good reason to be so restrained in the application of the Canon. Allen-Hermanson, for example, writes that he “can see nothing incoherent about extending the Canon to include showing a preference for *nonmental* faculties over ones that are psychological.”²⁴ But it seems to me, on the contrary, that once one makes this step, certain difficulties at once present themselves. The reason is that if we interpret the Canon as endorsing a preference for non-psychic over psychological explanations, it will almost certainly end up by licensing far too much. A first attempt at expressing the worry here might go as follows: it is *always* possible to offer non-psychological explanations of patterns of behavior, alongside the psychological

separate matter and has nothing to do with the question of defining instinct in a manner which shall be formally exclusive, on the one hand, of reflex action, and on the other, of reason.” (*Animal Intelligence*, 11-12) There is every reason to suppose that Morgan would have adopted the same view. For him, explanations adverting to animal instincts were psychological explanations implying the presence of consciousness – and the hierarchy of powers to which his Canon makes implicit reference is hence an entirely *intra-psychic* hierarchy.

²³ Morgan, *An Introduction to Comparative Psychology*, 124. My italics.

²⁴ Allen-Hermanson, “Morgan's Canon Revisited”, 613.

ones which we might also think correct. This is true even in the human case. For example, I (and millions of other human beings) leave our homes each morning for work where we intend to earn the money to support ourselves and our families. To mention the aim of earning money is to offer a psychological explanation in terms of intention and purpose – making the activity into one which can be readily be understood as a prime case of the operation of practical reason. But there will also be a range of lower-level explanations available of this widespread variety of human behavior. There will, for example, be a physiological account to be given of the causal origin of the walking movements we make in leaving (and in opening doors, stepping outside, closing our doors behind us, setting off down the road...) in terms of activity in the motor cortex, responses to incoming perceptual data, perhaps the setting in train of certain habitual mechanisms (we know without looking where our own door handle is, what sort of force will be needed to turn it, and so on). And yet we do not normally suppose that the existence of the second sort of explanation excludes the relevance and importance of the first. Yet Morgan's Canon, extended in the way envisaged by Allen-Hermanson, might seem to advise us to dispense with the intentional explanation entirely, in favour of the purely physiological one.

It might be said in response – and it would be rightly said, in my view – that this argument is too quick. For it assumes that the physiological explanation in such a case would be an explanation *of the very same thing* as the intentional one – and that this is not in fact the case. The psychological explanation explains why a certain pattern of intentional activity has occurred – whereas the physiological explanations merely explain *why such and such bodily motions took place*. We have here two explananda and not one, and that is why two distinct explanations are admissible. But so far as animals are concerned, the very thing which is likely to be in question, when an application of Morgan's Canon is envisaged, is whether one can distinguish in this way or not between similarly differentiated explananda – whether, for example, there really is, in a given case, something it might make sense to think of as an intentional action present, in addition to a mere set of movements, or not. Is the Australian jewel beetle trying to mate with the beer bottle which it mistakenly believes to be a female? or should we simply say that certain features of the bottle have triggered (in an entirely mechanistic way) the 'release' of certain behaviors? We face in such cases as this a question about how the general phenomenon, an instance of which we are currently observing, is to be *described* before we can begin to even approach the question how it is to be explained. It is simply not the case, as the formulation of Morgan's Canon rather takes for granted (in speaking of 'an action' which is 'the outcome' of a given process), that we can offer an entirely neutral description of some single pattern of activity which then might admit either of a psychological or of a non-psychological explanation. As Dennett has often argued, what we face in these cases is rather the more general, holistic question of whether there is a psychological *pattern* to be found across what is perhaps quite a wide range of behavior in different kinds of context, a psychological pattern which would be explanatorily and predictively helpful to us in our quest to interpret the world – and the answer to this question will dictate the description of the activity or behavior we want explained in the first place, as well as the form of the explanation itself.²⁵ As Dennett sees it, the crucial question is this: does the intentional stance pay significant dividends in the attempt to understand the overall workings of a certain organism, or not? And it is not really clear how Morgan's Canon is to be applied, once this difficulty is recognized. If we interpret it as telling us always to favour a non-intentional, more mechanistic stance wherever we can do so – to look upon a creature as a mere mechanism wherever that is possible – we seem to be in danger of outlawing psychology altogether. For the trouble is that in a certain sense, it is always *possible* to take a mechanistic stance to a pattern of activity. We can always decide (even in the case of human action) that our explanandum

²⁵ Daniel Dennett, *The Intentional Stance* (Cambridge, MA: MIT Press, 1989).

is merely a set of bodily motions and resolve to seek merely to explain *those* in terms of their proximate neurological causes. But presumably we (mostly) think we would miss an utterly enormous amount, were we to restrict ourselves in this way, in the human case. Why, then, think that the decision to do so is not similarly likely to lead us astray in the case of other animals?

It might be said that this conclusion would only follow if we overlook Morgan's own clarification that we are allowed to offer a 'higher' process in explanation of a phenomenon *in a case where we have independent evidence of some sort that it exists*. That is what we have in abundance, it might be said, to justify the application of psychological interpretations in our own case and perhaps we might be happy to concede its existence (in the form of hugely increased general explanatory and predictive power) also in the case of certain other higher animals, such as primates and dogs. It might be doubted, I imagine (and I would myself doubt), that we have such independent evidence in the case of the jewel beetle. Morgan's Canon is to function, it might be insisted, merely as a "tie-breaker", to be brought in only where, having taken all available evidence into account, there *still* seems to be no clear reason to prefer the adoption of the intentional stance to a non-intentional or even entirely non-psychological form of explanation. It is merely a directive that takes effect, other things being equal. But there would be two serious worries about trying to defend Morgan's Canon on this more limited basis. One is that one might reasonably have the suspicion that the number of cases in which all else really *is* equal is likely to be vanishingly small – indeed, that it might end up being a null class. A situation in which even when all the evidence that could possibly be relevant to the interpretation of a piece of behavior is taken into account, there might be *nothing* which tips the balance in favor of the view that one or other of the competing hypotheses is the more probable, is hard to imagine. The other is, of course, the question already raised of what would *justify* the use of the Canon in such a case, to break the tie – of why agnosticism would not be the proper response instead. I shall turn shortly to consider some possible answers to this important question. But first, having cast doubt on the idea that it is safe to extend the Canon beyond Morgan's own intra-psychic application, let us return to the question I raised earlier, namely, the question how we might interpret the difficult terms "higher" and "lower".

(iii) "Higher", "Lower" and the Psychic Hierarchy

As I have already explained, Morgan was operating with a basically tripartite understanding of the psychic "faculties" which might underlie animal behavior – instinct, intelligence and reason – though he seems to have been perfectly aware that this division was strictly *pro tem* and likely ultimately to be superseded by a more complex set of divisions, particularly within the vast category that he calls "intelligence". Undoubtedly, Morgan's own suggestion that these powers can be arranged hierarchically was underwritten, and perhaps made to seem more obvious than it really is, by assumptions which are false, in particular, the assumption that evolution "always marches from simple to complex," as Sober puts it, and the vestiges of the linear conception of a scale of nature which Darwinism refuted, according to which every species can be ranked as "higher", "lower" or "equal to" each of the others. In particular, we must suspect Morgan of having taken for granted the thought that "higher" implies "more characteristic of, or closer to, distinctively *human* behavior" and hence the assumption that man is the measure of all things cognitive. From a less anthropocentric viewpoint, one might indeed be tempted to claim, as Sober suggests at one point, that "it makes no sense to ask whether the ant's use of pheromones is higher or lower than the plover's use of deception. Both have their place in life's diversity."²⁶

²⁶ Sober, "Morgan's Canon", 225.

But even if we must beware of anthropocentrism in the very establishment of the scale in terms of which we are to understand Morgan's Canon, it is surely not true that we can make *nothing* sensible of the idea that some interpretations of creatures' behavior are more sophisticated than others. It is not, of course, that the operations of instinct are any less *impressive* than the workings of reasoned thought; indeed, in some ways, the reverse is true. It is *astounding* that a spider can spin a web or a bird build a nest without practice, without forethought, without teaching. Nor is the concept of *complexity*, I suggest, the most useful way to think about the question which powers are 'higher'. There are just too many different ways in which to measure complexity for this to be a very useful suggestion. The processes behind the operation of many instincts, for instance, must be staggeringly complex, and in some ways might even seem to exceed in complexity the sorts of simple logical operations that are involved in some of the most basic varieties of reasoning.²⁷ What makes us regard certain interpretations of behavior as "higher", I suggest, is what they seem to imply about the overall capacities of the behaving *agent*. Where a behavior is attributed to instinct, comparatively little is left for the operation of judgment, decision or thought to settle at the time of action; and few resources are presupposed which require such things as the consideration and weighing of *alternative* means to given ends. A spider may have to find a reasonable place to build her web – but web-spinning itself is an ability she will not need to have learned or have practiced, and which we can think of, more or less, as involving simply the execution of an entirely domain-specific programme with which the spider comes ready-equipped. Some degree of flexibility and adaptability is doubtless required but the calls on the spider-agent, deliberating in the moment, to come up with spur-of-the-moment solutions to unforeseen difficulties are plausibly not many. And should the spider for some reason be unable to construct an effective and successful web, she is unlikely to come up with many ideas about what she might do instead to get a meal. She will not, for example, be inventing wholly new kinds of fly trap or discussing interesting alternative possibilities with her conspecifics. If an instinctive behavior cannot be made to do the job for which it is intended (and no 'back-up' mechanisms have been selected for), then that is more or less that.

Moving up into the realms characterized by what Morgan called "intelligence", there is likely to be more flexibility and adaptability on show. New Caledonian crows can seemingly put together various parts of mechanisms they have learned to manipulate in order to reach food in such a way as to hatch a successful plan when confronted with a new and more complex mechanism than any they have hitherto seen. This cannot be explicated by means of instinct alone; the situation they face is new and requires immediate, on-the-spot assessment by something which it is very natural to suppose must be a kind of thinker. But we need not suppose the existence of thought which is in all respects like our own, in order to make sense of the postulated abilities. We need not attribute, for example, such things as episodic memory, arithmetical abilities, a sense of self, or conceptual thought, at least, not where we have no other behaviors to go on. What we attribute is a level and type of intelligence of the sort necessary to explain the kind of problem-solving revealed by the crows: no more.

Reason – a category as unsatisfactorily vague in its own way as 'intelligence' – is at the top of Morgan's hierarchy, I think, for several reasons. One is obviously its traditional place as the differentia of the human species and its traditional associations with the presence of linguistic abilities, via the concept of *logos*. Another less anthropocentric justification one might offer for thinking that it truly belongs there is that 'reason' is often used to refer to a set of abilities to manipulate information which are potentially entirely domain-general; inferential abilities, for example, can be applied, in principle, to problems which share the same logical shape, no matter

²⁷ As Fodor points out, all you need to make a Turing machine, in a sense an excellent reasoner, is "two kinds of pebble and an endless roll of lavatory paper" ("Not so Clever Hans", 13).

what the topic of the reasoning involved. It is true that evidence suggests that we cannot always exercise our reasoning abilities on some kinds of problem as well as we can on others – there may be more domain-specificity in human thinking that has hitherto been recognized.²⁸ But still, the applicability of general logical principles and mathematical reasoning, etc. to diverse puzzles about the construction of buildings, the calculation of prices, the drawing of maps, the prediction of the motions of planets, etc., is striking, and there seems to be little evidence of such very diverse applications of inferential thinking in creatures other than ourselves. We are probably less inclined than Morgan would have been to suppose that evidence of the application of reason is necessarily evidence of an agent consciously thinking, for unlike Morgan, we have seen what mere machines are able to do in the realm of calculation, inference and problem-solving and how domain-general are the abilities they display; and perhaps less likely also to think that reason and language necessarily go hand in hand. But the presence of the two together certainly appears exponentially to expand the range of a creature's abilities. We may additionally associate with reason such things as the ability to think about things in their absence; to engage in counterfactual thinking; to think utterly freely about future and past; to form new concepts and categories, building on those which already exist – all things which seem to have been crucially important in human development and to have put within the reach of their possessors a range of capacities to act on the world of which no other animal has so far been capable. Morgan's categories are doubtless too broad, too vague and too all-encompassing: but it cannot be said that we have no idea at all what to make of the suggestion that some processes offer "higher" interpretations of creatures than others. We can at least make rough-and-ready sense of what is at issue – enough sense, anyway, to make Morgan's Canon definite enough in its specific directives to be practically usable.

Suppose, then, that we adopt, roughly, the suggestion offered above as to how we are to understand the terms 'higher' and 'lower'. "Lower" processes carry with them fewer commitments to further abilities; they are less flexible in the face of unexpected failure or alteration in circumstance; they are more domain-specific. Is there any reason to suppose that if we have no empirical evidence to prefer an interpretation of animal behavior in terms of "higher" processes to one in terms of possible "lower" processes, we should endorse the lower? I want now to consider whether there is anything to be said for Morgan's Canon, thus understood.

(iv) *The Justification of Morgan's Canon*

There seem to be roughly three kinds of argument available for the possible justification of Morgan's Canon. One variety of argument – perhaps the most common – suggests that it is simply a version of some very widely endorsed scientific principle of theory choice: either a principle of *parsimony*, or perhaps a principle of *simplicity*. A second suggests that it is a principle which has a specifically biological grounding of some sort. A third idea is that it is required in order to correct for an inherent bias toward anthropomorphism. All these possible justifications have been thoroughly worked over and in my view successfully demolished in the literature. I do no more here than simply summarize and modestly supplement points that have already been very clearly made by others.²⁹

The idea that Morgan's Canon can be justified on grounds of parsimony or simplicity of course raises the prior question how such generic principles of theory choice are themselves to be

²⁸ See Lawrence A. Hirschfeld and Susan A. Gelman, *Mapping the Mind: Domain Specificity in Cognition and Culture* (Cambridge: Cambridge University Press, 1994), for a discussion of the phenomenon of domain-specificity.

²⁹ Notably Sober, "Morgan's Canon"; and Fitzpatrick, "Doing Away with Morgan's Canon".

justified, but let that pass. Some of those who have invoked the idea that the Canon is simply one application of such a principle have been willing to bite the bullet and accept that such principles cannot be justified but must simply be assumed.³⁰ But it is *not* a parsimony principle; and though it seems to me arguable that it *is* a variety of simplicity principle, there are many kinds of simplicity, and very much more argument than has been offered would be needed to support the view that the kind of simplicity it approves must be preferred over the other varieties. Parsimony first: there is more than one kind of parsimony one might have in mind in putting forward a parsimony principle, for example, straight ontological parsimony, of the sort advocated by Ockham's razor; parsimony of theoretical assumption; cladistic parsimony (i.e. parsimony of separate evolutionary origins for a given trait). But parsimony is always about getting by with *fewer* of something; it is not about getting by with "lower" rather than "higher" faculties and processes.³¹ Simplicity, admittedly, is a different virtue from parsimony; perhaps a preference for interpretations in terms of 'lower' processes does indeed amount to a preference for one kind of simplicity. But as Fitzpatrick and Fodor both convincingly argue, there are many varieties of simplicity, and we will still need to know how to trade off the different kinds one against another. To take a nice example from Fodor³², on one conception of simplicity, the simplest way to play tic-tac-toe is to memorize all the possible games and play by rote. But that is not the way anybody in fact plays, nor would it be sensible to programme a machine to play that way, since there are rule-based solutions that work just as effectively, but use less computational power. If "rote learning" is "lower" than the application of rules, as might seem intuitive, Morgan's Canon looks as though it would suggest the wrong result in a case like this. To take another case from Fitzpatrick, it is plausible that it is "simpler" in one sense to suppose that we should use similar psychological explanations for similar behavior, where the behavior is found in more than one species.³³ But suppose we had abundant evidence that in one of these species, the behavior was produced by way of a "higher" process? Morgan's Canon would seem to insist that we prefer the "lower" process as an account of what is going on in the other species, nevertheless; but it is not in the least obvious that we ought to do so.

Morgan's own attempt to justify the Canon does not go by way of simplicity; indeed, Morgan assumes that simplicity would dictate utilising the most anthropomorphic explanation available. His own justification appears to invoke the idea that the psychic powers arrayed in his hierarchy evolve from one another, and hence looks to be intended as a specifically biological justification:

<ext> "... it is clear that any animal may be at a stage where certain higher faculties have not yet been evolved from their lower precursors; and hence we are logically bound not to assume the existence of these higher faculties until good reasons shall have been shown for such existence".³⁴ </ext>

But as Sober argues, this is not enough to justify the Canon. There is no reason to suppose, even if it were true that the higher faculties were evolved from the lower, that the lower ones are more likely to explain any given piece of animal behavior than the higher. The evolution of the higher capacities

³⁰ See e.g. Robert Epstein "No defense of the principle ... [of parsimony] ... is offered, for ... I believe that no definitive defense is possible ... I simply assume it, as did Ockham and others, as a first principle ..." ("The Principle of Parsimony," 119.

³¹ This point appears to have been first made by E. Newbury, "Current Interpretation and Significance of Morgan's Canon", *Psychological Bulletin* 51, 1954, 70-74: 72, who is quoted approvingly by Sober, "Morgan's Canon", 230.

³² "Not so Clever Hans", 13.

³³ "Doing Away with Morgan's Canon", 231.

³⁴ Morgan, "An Introduction to Comparative Psychology", 59.

might well involve the atrophy of the lower: faced with a given pattern of activity, then, there is no prior probability that it is a lower rather than a higher capacity that explains it.

Someone might think that the best justification for Morgan's Canon is that it guards against an opposing bias which is present in all of us – a bias toward anthropomorphic explanations and interpretations. I think it may well be true that human beings do have such a bias. We are good at searching for mindedness in the world and it is undeniable that we have historically found it in many places where it turned out not to exist, inventing gods and demons, spirits and fairies, ghosts and goblins, to explain phenomena that did not need to be thus explained. But in general, it is not true that the best way to compensate for one bias is to introduce another. Evidentialism alone will do the trick, and is far safer. All that is needed, when a suspiciously anthropomorphic explanation appears to be on the cards, is the caution that other possible explanations of the behavior in question may need to be investigated, and empirical reasons found for preferring one of the rival explanations over the other.

Might it be objected, though, that evidentialism faces difficulties of its own? One might worry, perhaps, that "higher-level" explanations of any given pattern of observed animal behavior are always available in principle, and that without Morgan's Canon, there is simply no way to discard these explanations, despite what might be alleged to be their intuitive extravagance. Consider, for example, a behavior of which we might suppose ourselves to have at least the basis of an entirely satisfactory, relatively "low-level" explanation – say, the tendency of worker ants from the same colony to respond effectively and instantly to danger to the nest, and in particular to its queen and larvae. In cases of serious danger, the whole nest is sometimes moved in response to an attack which cannot be successfully repelled by soldier ants, with worker ants carrying the vulnerable larvae to a safer place. The role of instinctive, pheromone-guided behavior in eliciting appropriate responses from individual worker ants has been intensively studied, and much scientific literature now exists on how these processes work to ensure the protection of the colony.³⁵ But of course it would be possible to envisage other, "higher level" explanations of the same behavior. Perhaps, for example, someone might suggest that the response is mediated by emotion, by love for the colony, say, on the part of the ants, who then respond protectively in the light of their love. And without Morgan's Canon, it might be wondered whether there is any way decisively to rule out a suggestion that might strike some as intuitively absurd.

One possible riposte here would be to try to argue that evidentialism could only lead to the suggestion that we ought to suspend judgement about this case if an inappropriately restrictive interpretation of what could constitute a 'reason to prefer' one theory over another is adopted. In this particular case, perhaps, one might expect that the 'love' hypothesis could be rendered deeply improbable by further experimentation. But even if no such experiment could be devised, the point could be re-made that reasons to prefer one theory over another may relate not only to observations which are strictly of the specific behavior to be explained, but also to far more general considerations – such as, for example, the necessary conditions for the attribution of emotions such as love to a species of creature. These might involve a much more holistic view on the overall active repertoire of a given species of animal, and indeed, might perfectly well include philosophical, as well as scientific considerations. It seems plausible, for instance, that to attribute the capacity for love to a creature one would require evidence not only of a well-organized behavioral response to an attack on the purportedly loved entity, but also of distress in response to the death or destruction

³⁵ See e.g. N. Fujiwara-Tsujii, N. Yamagata, T. Takeda, and M. Mizonami, "Behavioral responses to the alarm pheromone of the ant *Camponotus obscuripes* (Hymenoptera: Formicidae)", *Zoological Science* 23 (4), 2006: 353-8; Makoto Mizunami, Nobuhiro Yamagata and Hiroshi Nishino, "Alarm Pheromone Processing in the Ant Brain: An Evolutionary Perspective", *Frontiers in Behavioral Neuroscience* 4 (28), 2010: 1-9; Tristran D Wyatt, *Pheromones and Animal Behavior: Chemical Signals and Signatures* (Cambridge: CUP, 2014).

of that entity; of specifically emotional forms of attachment, such as comforting and touching, for example; and an altogether more flexible repertoire of response to different kinds of threat to the survival and wellbeing of the larvae than ants are capable of displaying. Another possibility would be to invoke neurological evidence of a general kind relating to the supposed neural basis of the capacity for emotional response. At any rate, it seems very unlikely to me that there are cases in which *no* such general considerations would be available to provide reasons to allow one hypothesis to prevail over another. But in cases in which there truly were no such considerations, arguing for a reason to prefer the lower-level explanation over the higher, I would be inclined to stand by the verdict of evidentialism. If no reason, either scientific or philosophical, can be given for supposing that insects such as ants do not feel emotions, then we should remain agnostic for the time being, until such time as reasons can be brought forward for preferring one explanation over the other. The danger otherwise is that in attempting to adopt a bias thought to be opposed to our natural anthropomorphic tendencies, we shall underestimate some of our fellow-creatures.

Moreover, when a bias is embraced by the scientific establishment in some discipline, things are likely to go particularly badly wrong. The very experiments which might serve (in a world where evidentialism held sway) to undermine the bias are likely to be discouraged; and if undertaken at all, their results may be treated with undue scepticism. In 2008, Franklin D McMillan was still able to write that “it is still politically wise to present oneself as soundly behaviorist, if not radically so.”³⁶ The particular bias in question, moreover, in this case may interact in dangerous ways with other societal forces. There may be a gender component to anti-anthropomorphic sentiment in science; one can readily enough imagine it being supposed that it is foolish and oversentimental women who attribute mindedness to animals, and only men who are able to maintain a truly objective attitude. And human self-interest may also play a role. If animals are complex automata, we do not have to worry so much about agricultural methods, animal experimentation, the destruction of habitat, and so on. We have to be on our guard against the possibility that what masquerades as a sober methodological imperative in fact simply enshrines the prejudices that are most useful to us.

Is it mere speculation to suggest that Morgan’s Canon may have been not merely unneeded in psychology (given the availability of the evidentialist alternative) but could also prove actually to have been damaging? I shall offer here, to conclude my discussion, two examples of cases which shed light on this question, in different ways. The first is a case discussed by Simon Fitzpatrick in his excellent critique of the Canon, a case in which the methodological imperatives of the Canon were ignored, to famously productive effect. The second is an instance in which they seem, rather, to have been followed – but where conclusions stronger than those really admissible appear to have been drawn in consequence, arguably as a result of Canonesque-style thinking.

(v) *Pernicious Effects*

If one believed in Morgan’s Canon, how might this affect one’s research strategy? Plausibly, it might affect one’s sense of what one’s working hypothesis ought to be, in any given case. As Fitzpatrick notes, it might make plausible what he calls the “scaling up strategy” – that “[i]n any given area of research into animal behavior, the working hypothesis should always be the least cognitively sophisticated explanation consistent with the available data.”³⁷ But Fitzpatrick claims that there are many examples from the recent history of animal psychology where this was *not* the working hypothesis – and where exciting results were discovered in consequence. The case he discusses in

³⁶ Franklin D McMillan, *Mental health and Well-being in Animals* (Hoboken, NJ: John Wiley & Sons, 2008), 72.

³⁷ “Doing Away with Morgan’s Canon”, 236.

detail is that of the discovery by Karl von Frisch of the famous honeybee dance, and its role in the communication of bees about the location of food sources.³⁸ Von Frisch discovered that the bees figure-of-eight-shaped “waggle dance” encodes three pieces of information: the direction of the food source (encoded in the angle of the waggle movements through the centre of the figure-of-eight relative to the vertical, which represents the angle of the direction in which the food is located, relative to the angle of the sun); (2) the distance from the hive of the source, encoded in the duration of the waggle runs through the centre of the figure of eight; and (3) the quality of the food source, encoded in the vigour of the dance.

Who would have dreamt that bees possessed such a sophisticated communicative device? In the years immediately following the publication of von Frisch’s findings, many were extremely sceptical of von Frisch’s claims, and critics defended an alternative “lower” hypothesis in terms of olfaction. If von Frisch had adopted similar views as his working hypothesis, it seems exceedingly unlikely that the significance of the honeybee dance would ever have been discovered. The exciting explanation he eventually offered of the ability of honeybees to use the waggle dance as a means of finding food, was only discovered as a consequence of his conviction that the dance was a communicative strategy, and his willingness to pursue this hypothesis with dedication. The lesson to be learned here, as Fitzpatrick suggests, is that researchers should be free to investigate whatever hypotheses suggest themselves, all things considered, untrammelled by *a prioristic* methodological assumptions. I turn now, to a second case in which the perils of the Canon can be discerned in a somewhat different way.

In a paper entitled “Distress Vocalizations in Infant Rats: What’s all the Fuss About?”, Mark Blumberg and his co-authors report on their investigations into ultrasonic vocalizations emitted by infant rodents on being separated from the nest. Such vocalizations had previously been interpreted by some investigators as cries of distress; Blumberg *et al* wanted to investigate the alternative hypothesis that the vocalizations were merely “acoustic by-products of a physiological manoeuvre, the abdominal compression reaction (ACR) that increases venous return to the heart when return is compromised.”³⁹ This had been suggested to them by the existence of two other contexts which are known to reliably elicit ultrasound production – extreme exposure to cold and administration of clonidine. Each of these two contexts produces a decreased cardiac rate, against which the ACR manoeuvre provides some protection. Their experiments revealed that administration of clonidine did indeed increase venal return to the heart (as well as ultrasound emission). These results, they suggest “provide strong, direct support for the ACR hypothesis, and, by doing so, underscore the potential pitfalls of anthropomorphic interpretations of the vocalizations of infant rats.”⁴⁰

If the ACR hypothesis is the hypothesis that the vocalizations produced by infant rats when removed from the nest are mere by-products of a physiological maneuver, we must ask what it means to say that the experimental results “provide strong direct support for the ACR hypothesis.” It is legitimate to say that *relative to the state of the evidence prior to these experiments having been done*, there is now more reason than there was previously to think the hypothesis is a possible explanation of the vocalizations – because we now know of another context in which ultrasound production and increased venous return go together. But that is *all* we can say. We cannot make any further judgements. The ACR hypothesis is somewhat more likely *than it was before* to be true; but we cannot say (on the basis of these experiments alone) that it is now more likely to be true than the

³⁸ Karl von Frisch, *The Dance Language and the Orientation of Bees* (Oxford: Oxford University Press, 1967).

³⁹ Mark S. Blumberg, Greta Sokoloff, Robert F. Kirby and Kristen J Kent, “Distress Vocalizations in Infant Rats: What’s all the Fuss About?”, *Psychological Science* 11 2000, 78-81: 78.

⁴⁰ Blumberg et al, “Distress Vocalizations in Infant Rats”, 78.

explanation in terms of emotional distress. So the results do not provide strong or direct experimental support for the ACR hypothesis *as against the distress hypothesis*. We are still entirely in the dark about which hypothesis offers the better explanation of the phenomenon observed.

In the light of this, what entitles Blumberg and his colleagues to make the claims they do about the perils of anthropomorphic interpretation? We have no reason whatever, as yet, to suppose that the explanation in terms of emotional distress is mistaken. We *do* have reason to think that we should, for the moment, be agnostic, until further enquiry has given us reason to think one or other explanation is definitely better; and perhaps Blumberg *et al* merely mean to make the point that other explanations for the infant rat vocalizations may be possible and should have been considered before the distress vocalization hypothesis had been endorsed. *That* is true enough and mere evidentialism would support it. But it seems to me that Blumberg and his colleagues mean to go further than this. In a follow-up paper, they suggest that their theory, which claims that “a common physiological event triggers ultrasound production regardless of the context or manipulation used” is more parsimonious than that offered by the distress hypothesis – a claim which in this context is perilously close to an invocation of the Canon, a plea to favour the “lower” process which seems to offer a good explanation in the clonidine case, over the “higher” which attributes an emotional state to the infant rats. But considerations of parsimony cut both ways. For we know that emotional distress is the reason why some infant animals vocalize, so there is a variety of explanatory parsimony one can draw on to support the “higher” explanation, too. Blumberg and his co-authors are right that “it certainly does not follow that all infant vocalizations are emitted for a communicatory purpose.”⁴¹ But once again, the correct response is agnosticism, and further investigation. One can draw, I suggest, no conclusions at all about the relative likeliness that ACR explains the ultrasonic vocalizations, as opposed to the hypothesis of emotional distress, from the research they cite.

(vi) Conclusion

It is almost impossible for an armchair philosopher criticising the practices and assumptions of working empirical scientists to avoid courting opprobrium. So perhaps it is necessary to say, in conclusion, that I hold no brief for anecdotalism, nor for inappropriately anthropomorphic interpretations of animal life; and I have the utmost respect for empirical methods. But one should not confuse the application of the Canon with the careful interpretation of empirical results. The role it has played, indeed, amounts, in my view, to a rejection of the cautious agnosticism that ought to be the proper response to an as yet empirically unsettled question as to what mental capacities truly account for a particular instance of animal behavior.

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⁴¹ Mark S. Blumberg and Greta Sokoloff, “Do Infant Rats Cry?”, *Psychological Review* 108, 2001, 83-95: 86.

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